

THURSDAY, FEBRUARY 7, 1878

THE SOCIETY OF TELEGRAPH ENGINEERS

WHEN a society which numbered 110 members at the date of its first public meeting can, after an existence of only six years, count 1,000 names upon its books, it has at least justified its existence, and those who have taken the chief part in calling it into being and guiding its course, may fairly consider that the numbers who have sought association with them prove that their proceedings have been, at any rate, not injudicious. It was, therefore, with good reason, that Dr. C. W. Siemens began his address to the Society of Telegraph Engineers on the occasion of his re-election to the office of President, by congratulating the society on the progress made by it since he addressed it in the capacity of its first President on February 28, 1872.

In these congratulations we heartily join, and we think that no one will question the wisdom of the society in calling back to the Presidentship a man who did so much in the early days of its career to prepare the way for the success since realised.

The claim of the Society of Telegraph Engineers to rank as a scientific institution cannot, however, be founded upon the mere number of its members, nor even on the scientific eminence of some of the names to be found in the list. Its scientific position must of course be judged of by considering, not how many or who its members are, but what they do in their associated capacity for the advancement of science. Ample materials for forming such an estimate as this are afforded by the six substantial volumes already published of the *Journal* of the Society of Telegraph Engineers. These volumes contain the papers communicated to the Society and reports of discussions at the meetings, and in addition a considerable number of reprints or abstracts of papers published elsewhere, bearing on the objects pursued by the Society. As might be expected in the case of a society founded primarily to promote the advancement, not of abstract science, but of a branch of industrial enterprise, papers of a so-called "practical" kind are the most numerous, and, if we may judge from the reported discussions, papers of this class are those which call forth the most general interest at the Society's meetings. But even among such papers, embodying as they usually do the results of careful observation and long experience on the matters of which they treat, there are few from which the student of physics may not gather some hint of value. There are, however, a considerable number of papers of which the scientific bearing is more direct. These are papers which, dealing with questions arising primarily out of the practice of the telegraph engineer, treat the problems discussed from the point of view afforded by the general scientific principles applicable to them, or which contain results of no less scientific than practical value. Among papers of this class, one by Mr. Hockin (vol. v. pp. 432-459) on "The Magnitude of Signals received through a Submarine Cable with various Connections at each end, and the best Resistance for the Recording Instrument," is specially deserving of mention. It contains a very masterly

treatment of what is in reality a purely scientific problem, though one which has very direct practical importance. And here we may digress for a little in order to point out that this paper of Mr. Hockin's affords an instructive illustration of the mutual beneficial interaction between "theory" and "practice" of which the whole history of the electric telegraph is full. The telegraph is in a fuller degree than most practical inventions the direct outcome of scientific investigation, but when, in the progress of telegraphic enterprise, the project arose of laying long submarine lines, it was found that, though the general nature of the electrical difficulties to be encountered was known, yet the scientific knowledge of the time was not sufficient to indicate clearly the way in which they were to be overcome, and from the nature of the case but little help was forthcoming from empirical experience. The matter was in this state when Sir William Thomson took up the question of the transmission of signals through submarine telegraph cables, and showed how the practical message-carrying power of an insulated conductor laid under water is connected with the dimensions and certain definite electrical qualities of the conductor and its insulating coating. The conclusions which he arrived at mathematically as long ago as 1855 have since remained the foundation of all successful practice in the manufacture of telegraph cables. Sir W. Thomson, however, took account only of the properties of the cable itself, whereas in the actual working of submarine telegraphy very much depends upon the proper selection and arrangement and adaptation to each particular cable of the sending and receiving apparatus employed at the two ends; and what Mr. Hockin has now done is to give a general theory which takes account of the electrical properties of the instruments as well as of the cable. Returning to the *Journal* of the Society of Telegraph Engineers, we may mention a short paper by Mr. Sabine on the Capacity of Accumulators Various Combined, one by Sir William Thomson on the Comparison of Electrostatic Capacities, and a note by Prof. Maxwell on the Theory of Lightning Conductors, among the original articles, as well as Messrs. Longridge and Brooks's paper on the Submergence of Telegraph Cables and Mr. Schwendler's on the Theory of Duplex Telegraphy, among the reprints, as examples which afford further proof that the Society, is established for practical objects, is not blind to the aid to be derived in the pursuit of those objects from the study of scientific principles. And although we do not suppose that all the 1,000 members study such papers as we have referred to with great eagerness, yet the mere fact of their circulation must do something to convince the most arrogantly "practical" man among them that ignorance is not in all respects a ground for thankfulness.

So far this flourishing society has professedly occupied itself only with telegraphy, but there are not wanting signs either in the *Journal* or in Dr. Siemens's address, of the difficulty of separating telegraphy from other departments of what may be called applied electricity. Thus, more than one paper has been read to the society upon the application of electricity to firing mines and torpedoes, an operation which, when successfully performed, generally results in causing the persons affected to dispense permanently with telegraphic communication, and Dr. Siemens devotes nearly a quarter of his address

to discussing the application of electricity for illuminating purposes, to the transmission of motive power, and in metallurgic processes. Recent improvements in the means of obtaining powerful electric currents seem to open up a prospect of such applications as those just mentioned, assuming in the near future greater practical importance than they have hitherto possessed, and it does not seem unlikely that, whether or not they think fit to assume the wider designation, the Society of Telegraph Engineers will have become a Society of Electrical Engineers.

G. C. F.

TAIT'S "THERMODYNAMICS" ¹

II.

Sketch of Thermodynamics. By P. G. Tait, M.A., formerly Fellow of St. Peter's College, Cambridge, Professor of Natural Philosophy in the University of Edinburgh. Second Edition, revised and extended. (Edinburgh : David Douglas, 1877.)

PROF. CLAUSIUS is himself the principal founder of the kinetic theory of gases. The theory of the exchanges of the energy of collections of molecules was afterwards developed by Boltzmann to a much greater extent than had been done by Clausius, and it appears from his investigations that whether we suppose the molecules to be acted on by forces towards fixed centres or not, the condition of equilibrium of exchange of energy, or in other words the condition of equality of temperature of two bodies, is that the average kinetic energy of translation of a single molecule is the same in both bodies.

We may therefore define the temperature of a body as the average kinetic energy of translation of one of its molecules multiplied into a constant which is the same for all bodies. If we also define the total heat of the body as the sum of the whole kinetic energy of its molecules, then the total heat must be equal to the temperature multiplied into the number of molecules, and by the ratio of the whole kinetic energy to the energy of translation, and divided by the above constant.

The kinetic theory of gases has therefore a great deal to say about what Rankine and Clausius call the actual heat of a body, and if we suppose that molecules never coalesce or split up, but remain constant in number, then we may also assert, all experiments notwithstanding, that the real capacity for heat (as defined by Clausius) is constant for the same substance in all conditions.

Rankine, indeed, probably biased by the results of experiments, allowed that the real specific heat of a substance might be different in different states of aggregation, but Clausius has clearly shown that this admission is illogical, and that if we admit any such changes, we had better give up real specific heat altogether.

Statements of this kind have their legitimate place in molecular science, where it is essential to specify the dynamical condition of the system, and to distinguish the kinetic energy of the molecules from the potential energy of their configuration; but they have no place in thermodynamics proper, in which we deal only with sensible masses and their sensible motions.

Both Rankine and Clausius have pointed out the importance of a certain function, the increase or diminution

of which indicates whether heat is entering or leaving the body. Rankine calls it the thermodynamic function, and Clausius the entropy. Clausius, however, besides inventing the most convenient name for this function, has made the most valuable developments of the idea of entropy, and in particular has established the most important theorem in the whole science,—that when heat passes from one body to another at a lower temperature, there is always an increase of the sum of the entropy of the two bodies, from which it follows that the entropy of the universe must always be increasing.

He has also shown that if the energy of a body is expressed as a function of the volume and the entropy, then its pressure (with sign reversed) and its temperature are the differential coefficients of the energy with respect to the volume and the entropy respectively, thus indicating the symmetrical relations of the five principal quantities in thermodynamics.

But Clausius, having begun by breaking up the energy of the body into its thermal and ergonal content, has gone on to break up its entropy into the transformational value of its thermal content and the disgregation.

Thus both the energy and the entropy, two quantities capable of direct measurement, are broken up into four quantities, all of them quite beyond the reach of experiment, and all this is owing to the actual heat which Clausius, after getting rid of the latent heat, suffered to remain in the body.

Sir William Thomson, the last but not the least of the three great founders, does not even consecrate a symbol to denote the entropy, but he was the first to clearly define the intrinsic energy of a body, and to him alone are due the ideas and the definitions of the available energy and the dissipation of energy. He has always been most careful to point out the exact extent of the assumptions and experimental observations on which each of his statements is based, and he avoids the introduction of quantities which are not capable of experimental measurement. It is therefore greatly to be regretted that his memoirs on the dynamical theory of heat have not been collected and reprinted in an accessible form, and completed by a formal treatise, in which his method of building up the science should be exhibited in the light of his present knowledge.

The touchstone of a treatise on thermodynamics is what is called the second law.

Rankine, as we have seen, founds it on statements which may or may not be true, but which cannot be considered as established in the present state of science.

The second law is introduced by Clausius and Thomson as an axiom on which to found Carnot's theorem that the efficiency of a reversible engine is at least as great as that of any other engine working between the same limits of temperature.

If an engine of greater efficiency exists, then, by coupling this engine with Carnot's engine reversed, it is possible to restore to the hot body as much heat as is taken from it, and at the same time to do a certain amount of work.

If with Carnot we suppose heat to be a substance, then this work would be performed in direct violation of the first law—the principle of the conservation of energy. But if we regard heat as a form of energy, we cannot apply

¹ Continued from p. 259.